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Abstract

Conditional cash transfer (CCT) programs aim to reduce poverty or achieve other social goals by making the transfers conditional upon the receivers' actions. Conditions are designed to encourage some desirable behavior that recipients might otherwise under-invest in. An unintended consequence of the conditionality may be to distort recipients' actions in ways that lower their welfare. The transfer size plays an important role in shaping such distortionary effects. In certain circumstances, a larger transfer increases distortion more than that it raises benefits from stronger encouragement, implying that (i) there is an optimal transfer size for CCTs, and (ii) unconditional cash transfers (UCTs) may be better than CCTs when the transfer amount is large. We illustrate a range of distortions arising from CCT programs around the world. We then introduce an experimental design that permits a test of this distortionary effect, and implement it in a cash transfer program conditional on seasonal labor migration in rural Indonesia. We find that when the transfer size exceeds the amount required for travel expenses, distortion increases and CCT program outcomes deteriorate.

Keywords: Conditional cash transfers, Distortion, Seasonal migration

JEL Codes: I38, O15, J48

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1 Introduction

Conditional Cash Transfer (CCT) programs started in the late 1990s in Latin America, and have become the anti-poverty program of choice in many developing countries around the world. They have grown from 3 countries in 1997 to 40 countries in 2010, to 64 non-OECD countries in 2014. Programs have now also been implemented in many OECD countries (Honorati et al., 2015; Medgyesi and Temesváry, 2013). CCTs are distinct from Unconditional Cash Transfer (UCT) programs in that they have behavioral conditions tied to them. Conditionalities attached to most CCT programs aim to encourage human capital investments. Examples of common conditions include school enrollment and attendance, health checkup visits of children and their vaccination (Dearden et al., 2009; Macours et al., 2012; Cahyadi et al., 2020). Other programs encourage positive environmental actions, such as leaving forest intact or planting new trees (Jayachandran et al., 2017; Jack and Jayachandran, 2019). Under a broader definition, CCTs include scholarships and workfare programs tied to participation in training or schooling, or supplying labor. Unconditional cash transfers (UCTs) are cheaper to deliver and administer because no monitoring of conditions is required. This leads to a fundamental tradeoff that policymakers designing transfer programs must grapple with: Are adding conditions to transfer programs and monitoring adherence worth it? Do CCTs improve welfare beyond UCTs?

The literature has examined the UCT-CCT tradeoff before (Baird et al., 2011; Attanasio et al., 2015; Akresh et al., 2013; Bergstrom and Dodds, 2020), but we add a new and important dimension to this debate: while CCTs may encourage desirable behaviors, it may also distort recipients' decisions into engaging in activities that may not be socially desirable. For example, the conditioning of cash transfers on adult's participation in a public works program may increase children's domestic work to compensate for their parent's absence, thereby reducing their school enrollment (Shah and Steinberg, 2019).

This article begins by illustrating examples of distortionary effects from a wide array of locations and contexts. Next we explore theoretical issues surrounding the tradeoff between the encouraging and distortionary effects of conditional cash transfers. In what situations would we want to impose a condition or constraint on a transfer, even if the beneficiary is rational and capable of making the best decisions for themselves? Are there market failures we are trying to correct with the condition? We analyze the role of the transfer size, which potentially influences both the encouraging and the distortionary effects of the imposed condition. We characterize situations in which larger transfers increase distortionary effects while diminishing benefits from stronger encouragement. Thus, even in contexts where CCTs are successful in addressing a market failure, distortionary effects can start dominating when the size of the transfer gets large. As a result, in those settings, UCTs may be preferable to CCTs when the transfer size is large relative to household income.

Next we introduce an experimental design that can quantify this encouragement-distortion tradeoff, and implement that design in the context of a seasonal migration encouragement program in rural West-Timor, Indonesia. In many rain-fed areas of the world, poor households experience seasonal deprivation during the pre-harvest period, and seasonal migration to a city for casual labor can alleviate this deprivation (Bryan et al., 2014). We randomly select some subsistence farming families in one of the poorest regions of Indonesia to receive either an unconditional transfer, or CCT payments (of varying sizes) that are meant to defray travel costs, conditional on the household agreeing to send an individual to a nearby city in search of employment. The migration condition of the CCT is designed to correct a market failure first identified in (Bryan et al., 2014), which is the lack of an insurance market to insure the risky migration outcome of having made migration expenditures but not finding a job at the destination. Poor households close to subsistence level may hence be too risk averse to engage in often-profitable seasonal labor migration, and our subsidy provides insurance against this downside risk.

We find that the CCT induces migration, and also increases migration season earnings relative to the UCT benchmark treatment. However, when the size of the transfer is increased beyond what is needed to cover migration travel expenses, distortionary effects prevail. For example, people who do not have the skills to succeed at the destination travel only to collect the CCT payment. Larger transfers induce negative selection into migration of low-return types, and program impacts deteriorate.

2 Illustrative examples of distortionary effects of CCTs

The literature on Conditional Cash Transfers reveals several demonstrated instances of the distortion of economic incentives leading to unintended and sometimes undesirable consequences. Table 1 lists several examples.

2.1 Education conditions

Conditioning the transfer on school enrollment of a specific child within the household can change intra-household resource allocation. Parents may choose to specialize in the education of the recipient, displacing the schooling of his or her siblings and reallocating the hours of child labor. The net effect of the transfer depends on both the expanded household budget set and these distortionary effects. A CCT program in Cambodia reduced school attendance among ineligible siblings of some program recipients (Ferreira et al., 2017). A Colombian CCT requiring school enrollment finds that girls spent more hours unpaid household work if they were not assigned to the program but their sibling was (Barrera-Osorio et al., 2008).

The tying of school attendance requirements to household's receipt of cash transfers may also affect the child's mental health. An RCT in Malawi that transferred \$4/month to families conditional on their adolescent daughters' school attendance improved the mental health of girls who already went to school at baseline (Baird et al., 2013). However, when transfers are increased to \$8 or \$10/month, making the girls the breadwinner of the entire family, their mental health can deteriorate.

Table 1: Examples of conditionalities in CCT programs and behavioral distortions arising from them.

Program & country	Conditionality	Distortion	References
Conditional Subsidies for School Attendance pilot in Colombia	Enrollment in tertiary education	Increase in tertiary enrollment only in short (2 years) programs in low-quality colleges	Barrera-Osorio et al. (2019)
Conditional Subsidies for School Attendance pilot in Colombia; CESSP scholarship program in Cambodia	Enrollment in school	Households enroll boys disproportionately; reduced enrollment of non-treated siblings (Cambodia and Colombia), who work more hours (Colombia) (intra-household substitution effect)	Barrera-Osorio et al. (2008) (Colombia); Ferreira et al. (2017) (Cambodia)
Helping Outstanding Pupils Educationally (HOPE) scholarship in the US	Merit-based scholarships tied to credits earned	Students choosing STEM subjects less often	Sjoquist and Winters (2015) Rumbaugh (2016)
Oportunidades in Mexico; *Takaful* in Egypt	Health check-ups of children by mothers	Confinement of women to caregiving roles: increasing their supply of unpaid domestic labor and reducing their supply of paid labor	De Brauw et al. (2015) (Mexico); El-Enbaby et al. (2019) (Egypt)
Ethiopia's Productive Safety Net Program (PSNP); India's National Rural Employment Guarantee Scheme (NREGS)	Participation in public works	Children substituting labor for adults who are induced by CCT to work (more) outside household (enterprise): Reduced school attendance and increased time spent on domestic work by younger girls (Ethiopia). Increased agricultural labor supply by older children (India)	Hodinott and Gilligan (2010) Zibagwe et al. (2013) (India) Islam and Sivasankaran (2015) Shah and Steinberg (2019) (Ethiopia)
Seasonal migration CCT in Indonesia	Seasonal migration	Increase in transfer size beyond amount needed for transport induces additional households to select into migration, who are less productive at the destination and have lower (or even negative) returns to migration	[this article]

A pilot in Bogota, Colombia sought to encourage tertiary education via a CCT. The program increased tertiary enrollment only in short study programs and in low-quality colleges, which suggests that the primary driver of individuals' study choice was to comply with the CCT condition to receive the subsidy, rather than the intended benefit of education itself (Barrera-Osorio et al., 2019). Merit-based scholarship programs impose a performance condition instead of attendance - a minimum performance level according to a pre-specified metric. The Helping Outstanding Pupils Educationally (HOPE) scholarship program in the US is an example of such a performance-condition. Performance conditions may lead to potentially undesirable distortionary effects if people can game program rules. In the HOPE program for instance, performance evaluation was based on number of credits obtained, leading students to reduce their enrollment in Science, Technology and Math (STEM) subjects, which are perceived to be more difficult (Sjoquist and Winters, 2015; Rumbaugh, 2016).

2.2 Gendered conditions

Cash transfers are sometimes targeted to women to support their empowerment by increasing resources available to them, or because mothers have a greater propensity to invest in the human capital of children than fathers (Hoddinott and Haddad, 1995; Quisumbing and Maluccio, 2003; De Brauw et al., 2014; Armand et al., 2020). However, conditions specific to mothers may risk reinforcing gender norms that women take on caregiving roles. It can reduce female labor supply especially when adhering to the conditions is time-consuming. Indeed, the famous *Progresa* program in Mexico that targets cash transfers to mothers if children receive health checkups, led to a reduction in female labor supply (De Brauw et al., 2015). A CCT program in Egypt also reduced labor supply outside of the household for female recipients (El-Enbaby et al., 2019). It should be noted that income effects from the CCT could also cause the reduction in labor supply.[1] Or, households may choose to change

[1]Banerjee et al. (2017) re-analyze data from seven randomized controlled trials of government-run UCT and CCT programs from six countries to examine impacts on labor supply, and find no significant effect either individually or pooled on employment or hours of work. Similarly, they find no pooled effect on

time allocation after investing the transfer in an enterprise. Indeed, the unconditional Human Development Grant in Ecuador reduced the propensity of transitioning to formal wage labor (Bosch and Schady, 2019), and a UCT program in Zambia increased labor supply on the own farm while reducing agricultural wage labor (Ervin et al., 2017; Prifti et al., 2019). Without the inclusion of a UCT comparison group in an (experimental) evaluation, it is difficult to assess to what extent empirically observed labor supply responses in a CCT program reflect distortions due to the conditionality, rather than generic behavioral responses to the cash component of the program.

2.3 Labor supply conditions

Where transfers are conditioned on an adult member participating in a public works program, the rationale is often that such public works have a public benefit (e.g., building roads, clearing debris after a natural disaster, or environmental conservation work). An unintended consequence can be to increase children's supply of domestic labor or farm labor, to substitute for the reduction in parents' labor. Such distortionary effects on child labor have been observed in the Productive Safety Net Program (PNSP) in Ethiopia (Hodinott and Gilligan, 2010; Zibagwe et al., 2013) and in India's National Rural Employment Guarantee Scheme (NREGS) (Islam and Sivasankaran, 2015; Shah and Steinberg, 2019). Workfare programs may also divert mothers' time from antenatal care, which in the case of NREGS, may have increased infant mortality (Chari et al., 2019). Given spatial differences in productivity and employment opportunities, some CCT programs encourage labor migration. Sections 4 and 5 of this article describe a seasonal labor migration CCT we ran and analyzed.

whether work is self-employed/within family vs. outside the household, nor do they find such effects when disaggregating by gender.

2.4 Distortions Induced by Program Qualification Requirements

When potential recipients are aware of program qualification conditions, they may alter economic behavior to meet eligibility criteria. For example, the Argentinian CCT programs, Programa Jefes de Hogar (PJH) and Universal Child Allowance for Social Protection (AUH), were targeted to the unemployed, and discouraged entry into formal employment (Gasparini et al., 2009; Garganta and Gasparini, 2015). Likewise, an Uruguayan CCT program reduced formal employment by 8 percentage points (Bergolo and Cruces, 2018). Brazil's Bolsa Alimentacao (Feeding Allowance) tied benefits to children being below a particular weight, and reportedly led to some children being kept underweight in order to qualify (Morris et al., 2004). If the size of the transfer is determined by the number of children, that can affect fertility. The Honduran CCT PRAF offered subsidies for health and nutrition to households with pregnant women or children under age 3, with a payment per child. The program increased childbearing in the short term (Stecklov et al., 2007). Such behavioral distortions can accompany either CCTs or UCTs that have program eligibility requirements.

3 Theoretical Considerations

3.1 Encouragement and distortionary effects of CCTs

When markets operate without friction, UCTs will dominate CCTs on efficiency grounds, since imposing a condition or constraint can only make the beneficiary (weakly) worse off. However, the rationale for tying conditions to cash transfers is the presence of market failures that lead individuals to under-invest in certain profitable and/or socially desirable behaviors. We impose conditions presumably to correct those failures.

Such market imperfections can arise from a variety of sources. First, some of the benefits of the encouraged behavior may be external to the decision-maker. This may lead to under-investment in behaviors that carry positive externalities (such as investments in the human capital of a child), or behaviors that reduce negative externalities (e.g. vaccinations, deworming, or environmental conservation) (Miguel and Kremer, 2004; Bärnighausen et al., 2014). Second, individuals may underinvest in behaviors or technologies with positive private returns due to a lack of information about those returns, or due to psychological biases such as procrastination, present-bias, or myopic behavior associated with poverty and risk (Carvalho et al., 2016; Mani et al., 2013; Laajaj, 2017). In some cases, an initial encouragement (e.g., through a CCT) may enable the decision-maker to learn about the individual-specific returns to an investment. Examples are an agricultural input whose effect on yield may depend on the specific soil characteristics of each farmer[2] (Foster and Rosenzweig, 1995; Gibbons et al., 2005; Zeitlin, 2011), or migration to a destination with a lack of information about the odds of finding work or about expected earnings there (Bryan et al., 2014). Beliefs about returns to investments such as education or migration may also be biased downwards if people who invested successfully hide or under-report their returns to friends and family, out of concern of being asked to share their returns with them (Jensen, 2010; Jakiela and Ozier, 2016; Baseler, 2020).

However, tying of conditions to transfers also distorts relative prices for a recipient. A program that transfers resources to mothers conditional on health checkups for their children reduces the relative cost of healthcare, but to the extent that these checkups are time-consuming, they may also divert time from earning labor income. Hence, compared to a UCT, there is a potential efficiency cost to CCTs.

3.2 The role of the transfer size

The size of a cash transfer plays an important role in the magnitude of encouragement and distortionary effects of CCTs, and therefore in determining the net effects of a CCT program. Consider the case of a CCT targeting a profitable behavior that individuals underinvest in due to a market failure, but where the benefits of take-up of the CCT and compliance with the

[2]Voucher programs that condition a subsidy on the purchase of specific agricultural inputs are common across Sub-Saharan Africa, see Holden (2019) for a review.

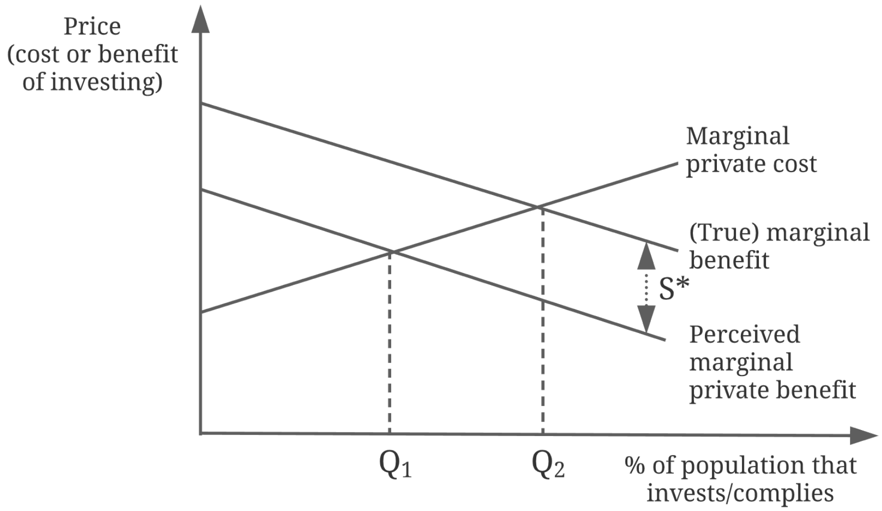

Figure 1: Individuals underestimate the returns to an investment targeted by a CCT (e.g., education, labor migration). A CCT of size S^* corrects this market failure; increases in the transfer size beyond S^* generate distortion.

condition are private to the recipient. Those benefits vary across individuals. For example, benefits of education or migration can vary with skill sets, or benefits of health checkups depend on pre-existing conditions. We assume all takers of the CCT comply with the imposed condition, and cost of compliance also varies (e.g. opportunity cost of school enrollment can differ across households). Individuals with the highest benefits and lowest cost of compliance would select into the CCT program. When the size of the transfer is increased, additional individuals with progressively lower returns and/or higher costs would choose to participate. As a result, the marginal private benefit is a downward-sloping function of the share of the population complying with the condition, and the private cost is an upward-sloping function (Figure 1). Selection into treatment on the basis of individual-specific expected gains is also referred to as essential heterogeneity (Heckman et al., 2006).

The optimal CCT subsidy size is S^*. As in the case of Pigouvian analysis, the transfer equals the extent to which people underestimate private returns (or the size of the externality), so as to equalize the true marginal social benefits of investing with the marginal cost of compliance. Transfer sizes exceeding S^* generate distortion, with larger transfers creating greater distortion and reducing the welfare effects of the program. Hence, for large enough transfers, UCTs can dominate the welfare effects of a CCT of the same size, even in the presence of a market failure.

Note that our analysis implicitly assumes that the private benefits and costs to each individual are independent of the transfer size. Larger transfers worsen the composition of the pool of compliers, but do not generate additional benefits per complying recipient. In some situations, larger transfers could enable recipients to make more profitable investments. For example, a larger scholarship may allow the recipient to attend a better college if those are more costly, or a larger migration subsidy may allow travel to cities farther away that offer higher wages. Larger transfers may also allow for more search, enabling a better employment match.

Larger transfers could also help address the adverse distortionary consequences of CCTs in some circumstances. For instance, when a CCT for school enrollment of a child of a specific age or gender displaces a sibling's education, a large enough CCT may enable it to pay for the schooling of the un-treated siblings. On the other hand it could intensify the distortion too. In a workfare program that diverts parents' time and attention away from children, larger transfers increase resources available for dowry, which increased child marriage rates (Tsaneva and O'donoghue, 2019).

In the next section, we apply this conceptual framework to design a CCT for seasonal migration, and test some implications of essential heterogeneity. Our experimental design varies the size of CCTs and compares them to a UCT benchmark as a way to quantify the encouragement-distortion tradeoff embedded in conditionalities.

4 Data and experimental design

4.1 Experimental context

In agrarian areas around the world, labor demand and wages fall during the pre-harvest season, and the prices of staples tend to rise while the economy waits for the new crop to grow. These combine to produce pre-harvest seasonal poverty and hunger (known as the 'lean season') in many poor rain-fed parts of the world (Bryan et al., 2014; Dercon and Krishnan, 2000; Jalan and Ravallion, 2001; Khandker and Mahmud, 2012; Macours and Vakis, 2010; Paxson, 1993; Fink et al., 2020). Rural areas of Eastern Indonesia experience such seasonal deprivation. In West-Timor the pre-harvest period is known as 'musim lapar biasa' (ordinary hunger period), which sometimes turns into famine-like conditions (known locally as 'paceklik') (Basu and Wong, 2015). Some rural households send seasonal migrants to cities to cope with this seasonal income shortfall. Our experiment is designed to test whether more households would benefit from employing the migration strategy, but are currently constrained from doing so. Appendix A provides additional details about the setting and the experiment, including the cropping calendar in West-Timor and the timing of our intervention and data collection activities.

4.2 Sampling

Five villages in Timor Tengga Utara (TTU) Regency in West-Timor were sampled based on poverty incidence and seasonality. Please see Appendix A for details on village selection. Out of 869 sampled households in these five villages, 855 gave consent to be interviewed and 775 of them satisfied the eligibility criteria of (i) having at least one household member aged 21 or above; and (ii) not owning land exceeding 200 Are (2 Hectare). Out of the baseline sample, 709 households (91%) were re-interviewed at endline, which took place from December 2017 until February 2018. Sample attrition does not differ statistically significantly across treatment arms (Appendix Table 4).

4.3 Experimental design

Randomization was done at the household level. First, households were randomized into either a UCT or a CCT treatment (Table 2). If a UCT-assigned household took up the offer, it received IDR 150,000 (∼USD 10), and no condition was imposed. Households assigned

Table 2: Treatment arms (amounts in Rp.)

	CCT			UCT (D)
	High (A)	Low (B)	Low with surprise (C)	
1st disbursement at the origin	150,000	75,000	75,000	150,000
2nd disbursement at the destination	150,000	75,000	(75,000 +150,000) =225,000	0
Total subsidy	300,000	150,000	300,000	150,000

to the CCT arm had the choice to take up the offer and migrate (to a destination of their choice within West-Timor), or to not take up the offer. The CCT payment was divided into two installments: Half paid at the village of origin after the offer is accepted, and the other half collected at the destination city after "checking in" with a program officer. This helped us monitor adherence to the conditionality.

The CCT arm was further split into three groups with varying transfer amounts, to understand distortionary effects through differential selection of migrants. A group we label 'CCT-high' received IDR 150,000 at the origin, and an additional IDR 150,000 after checking in at the destination (IDR 300,000 total). People randomized into a 'CCT-low' group received IDR 75,000 at the origin, and they were told they would get an additional IDR 75,000 upon checking in at the destination. Hence, their total disbursement of 150,000 equalled that of the UCT group. This CCT-low group was further split into two, whereby half of the households assigned to CCT-low at baseline who check in at a destination, were 'surprised' upon checking in to receive a second subsidy of IDR 225,000 rather than IDR 75,000. We label this group 'CCT-low+'. They also received IDR 300,000 in total, like the CCT-high group.

Doubling the promised transfer from 150,000 to 300,000 changes the selection of households who migrate, and that is the potential distortion that our research design was meant to capture. However, the larger transfer may also cause ex-post actions (e.g., searching longer for a job due to having a larger buffer, investing in household enterprises), which confounds the selection effect we are interested in. This is why the 'surprise' component in CCT-low+ is useful: The total transfer was 300,000, so it controls for the direct income effect, but since those households did not know that they were going to receive this when they made their offer take-up and migration decisions, we are able to capture the pure effect of the selection or distortion. We do so by comparing the effect of assignment to CCT-high or CCT-low+ on ex-post outcomes such as migration season household income. The ex-post 'surprise' treatment arm was inspired by Karlan and Zinman (2009), who offered different interest rates before and after loan applications are made to experimentally disentangle adverse selection from moral hazard in a credit market.

Appendix Table 4 shows that the treatment arms were generally balanced at baseline, but we show results with and without controlling for baseline covariates. Given that randomization was done at the household-level, there is a possibility of spillover effects on UCT assigned households (for example, by inducing some UCT assigned neighbors to (co-)migrate). The direction of spillover effects apparent in Appendix Figure 7 shows that the impact on income of the CCT (as compared to the UCT), if anything, is an underestimate of the true effect. Appendix B also contains a description of the construction of the variables used in our analysis.

5 Results

5.1 CCT versus UCT

Not surprisingly, the CCT subsidy is more likely to be spent on seasonal migration (to adhere to the condition), whereas the UCT subsidy is more likely to be spent on non-farm capital and food consumption (Appendix 4). Recipients of the CCT-low group receive the same total subsidy amount as UCT recipients. However, despite the significantly lower take-up rate in the CCT-low group (41.8%) as compared to the UCT group (93.8%), the CCT-low group dominates the UCT group in terms of the intent-to-treat effect on migration season income (columns (7)-(10) in Table 3; Appendix Figure 6). Since the emphasis of this article is on distortionary effects of CCTs, we now turn to the differences between CCT-high and CCT-low arms in columns (1)-(6).

Table 3: Impact (intent-to-treat effects) of assigned treatments on take-up of the offer, checking in at the destination, and household income during the peak of the migration season (the first two weeks of September).

	CCT subsample						Full sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:	Accepting offer		Check-in at a destination		Migration Season income (Rp. 10k)		Accepting offer		Migration Season income (Rp. 10k)	
CCT-high	0.050	0.066	0.105***	0.111*	-31.605	-23.441	-0.362***	-0.338***	8.268	8.401
	(0.045)	(0.038)	(0.040)	(0.051)	(20.173)	(14.020)	(0.039)	(0.0554)	(13.411)	(7.690)
CCT-low+					1.846	8.258				
					(23.126)	(7.456)				
CCT-low/low+							-0.418***	-0.401***	41.612***	36.602*
							(0.032)	(0.055)	(14.287)	(15.680)
Socio-economic status (SES)							-0.009	-0.010	14.867**	8.146
							(0.013)	(0.018)	(6.637)	(5.246)
SES × CCT-high							-0.068*	-0.055	4.169	6.870
							(0.041)	(0.054)	(13.040)	(5.829)
SES × CCT-low/low+							0.045	0.052	-18.203	-16.204
							(0.031)	(0.034)	(16.649)	(12.303)
F-test p-values:										
high = low+					0.077*	0.015**				
SES < median: high = low/low+							0.145	0.099*	0.011**	0.016**
SES > median: high = low/low+							0.875	0.683	0.406	0.434
E(dep. var. | CCT-low)	0.510	0.510	0.423	0.423	133.085	133.085				
E(dep. var. | UCT)							0.928	0.928	90.257	90.257
Controls		✓		✓		✓		✓		✓
Observations	526	474	526	474	474	474	775	708	708	708
R^2	0.002	0.031	0.014	0.036	0.008	0.067	0.163	0.169	0.020	0.080

* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors are clustered at the household level. Migration season household income is winsorized at the 99th percentile to ameliorate the undue influence of outlying observations. Controls include the head of household's gender and years of education, whether (s)he speaks Indonesian, household size, and a socio-economic status (SES) index, and village indicators. The F-test p-values for columns (7)-(10) are based on regressions of the outcomes on the CCT high and CCT-low/low+ assignment indicators where the sample is split between below-median and above-median SES households, instead of controlling for the interactions of treatment with the SES index. Details on the outcome variables and the construction of the SES index can be found in Appendix B.

5.2 Testing for Distortion

Providing the large subsidy raises the likelihood that households accept the CCT offer, and then significantly raises the rate at which migrants check-in with our program staff at a destination. CCT-high recipients are 11 percentage points more likely to check-in at the destination than the 'CCT-low'/'low+surprise' base check-in rate of 42% (columns (3)-(4) in Table 3).[3] No statistically significant differences between the CCT groups are found on the choice of destination, the duration of the migration, or on the propensity to find work (Appendix Table 6 and 7). The key question for us is whether this additional migration induced by the CCT-high treatment is "distortionary" in the sense that it induces a set of people to migrate for whom this is not really a productive activity. Simple accounting using our data suggests that this could be a problem, in principle. Migrants report their transportation expenditures, so we know that the CCT-low subsidy amount suffices for transport to common migration destinations (Appendix Table 6 and Appendix Figure 5). In other words, the extra transfer received by CCT-high assigned households exceeds their transport expenditure requirement, so it could induce recipients to travel just to collect the money, even if they are not especially well-suited or eager to find work at the destination.

Columns (5) and (6) (Table 3) indicate that this appears to be an issue. Even though the verified migration (destination check-in) rate is higher among CCT-high recipients, they report significantly lower migration season income compared to CCT-low+ households. The CCT-high vs CCT-low+ is the most relevant experimental comparison, because we hold the ultimate size of the transfer constant in these two groups, and only vary the selection. Column 6 shows that households that received the CCT-high offer have Rp. 32,000 lower migration season income (p-value = 0.015) compared to households that received the CCT-low+ offer. This difference is not driven by outliers (Appendix Figure 6).[4] The CCT-low+

[3] To the recipient household, CCT-low and CCT-low+surprise treatments are indistinguishable from each other until a household member checks in at a destination, which is why we only have one regressor (CCT-high) in columns (1)-(4), and the combined 'low' transfer is the omitted category

[4] No effects of any of the treatments was found on household food security during the hungry season at endline (Appendix Table 8), possibly due to the hungry season not coinciding with (coming after) the

group ultimately received the same-sized transfer as CCT-high, so we are holding constant any 'moral hazard' on labor supply caused by the monetary transfer, and only focusing on differences based on who selects in under each treatment.

5.3 Variation across Rich and Poor Households

If the large transfer indeed distorts people's decisions by inducing them to travel only to collect the transfer even when migration is not otherwise economically sensible, then we might expect this to be more of a problem among poorer households who are more desperate for this transfer. Columns (7)-(10) of Table 3 adds interaction terms between the assigned treatments and a measure of the household's socio-economic status (SES) at baseline to explore this heterogeneity. Higher values of the SES index correspond to higher household wealth. Please see Appendix B for details on the construction of this index.

Columns (7) and (8) show that naturally, take-up of the offer is highest in the UCT group (93%). Among the poorest households, take-up is a little higher for the CCT-high compared to the CCT low/low+ groups. The coefficients on two SES-interaction terms imply that as households get richer, the take-up difference between CCT-high and CCT-low disappears. Most importantly, columns (9) and (10) show that it's only among the poorest households in our sample that the CCT-high treatment generates significantly smaller migration season income. In the poorer half of our sample, migration season income is significantly lower when the large transfer is offered (p-value of difference = 0.011). The coefficients on the SES interaction terms imply that as we move up the income ladder in our sample, the gap between the effects of CCT-high vs low on migration income disappears. In the richer half of the sample, there is no significant gap (p-value = 0.41).

We interpret these results to mean that the relatively large size of the CCT-high transfer distorts the behavior of low-wealth households more. When offered a large subsidy, some additional low-wealth households are induced to accept the offer and migrate merely to collect

migration season in West-Timor (as described in Appendix A.

the subsidy at the destination, even if migration is not an economically sensible activity for that family. This lowers the overall effectiveness of the program under the CCT-high transfer.

6 Discussion

Policymakers face choices about whether to condition the transfers on socially desirable behaviors, about the type of conditions imposed, and about the transfer size. Theoretical considerations and credible empirical evidence can guide such decisions. A complete welfare accounting would include effects of the cash component, effects of adherence to the encouraged behavior that corrects the targeted market failure, distortionary effects of the imposed condition, distributional effects (changes in inequality), and spillover effects on treated and non-treated households. In this article, we focused on the distortionary effects on targeted households. Reviewing the literature of empirical impact evaluations of CCTs, we identified distortions brought about by CCT programs across a range of geographies, conditions, and target populations. Our conceptual framework focused on the role of the transfer size. It highlights that in certain CCTs, the distortionary effects increase in the transfer size, so that for large enough transfers, a UCT eventually outperforms a CCT, even when there is a market failure that the CCT corrects. Applying this framework to a seasonal migration CCT in Indonesia, we found that for increases in the transfer size beyond what is needed for recipients to comply with the migration condition, distortion increases and CCT program outcomes deteriorate.

These findings have several implications for the design of CCT programs and the design of evaluation studies. First, ex-ante theorizing about possible distortions generated by conditionality can inform the design of the condition, and nudge us to collect data on distortionary side effects (which may not be directly related to the primary outcomes). Second, the inclusion of a UCT comparison group allows a comparison of the extent to which observed behavioral responses reflect distortion due to the conditionality, rather than generic behavioral responses to the cash component of the program. Third, by experimenting with the transfer size, the policy maker can calibrate the amount that maximizes net program welfare effect given the encouragement-distortion tradeoff highlighted in this paper.

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Appendix A Context of the experiment in West-Timor, Indonesia

A.1 Village sampling

The RCT was conducted in West-Timor, in Nusa Tengarra Timur (NTT) province, Indonesia. West-Timor contains 5 of NTTs districts, plus the city of Kupang (the provincial capital city). The experiment was conducted in Timor Tenggah Utara (TTU) district, because of its seasonality and high poverty incidence. TTU district counts 24 sub-districts and 193 villages, and its capital city is Kefamenanu. We selected 5 villages in 5 different sub-districts, based on (i) seasonality, (ii) high poverty incidence (based on data from the Indonesian village census PODES), (iii) location near the border of the TTU and TTS regencies (and therefore a similar distance to So'e, Kefamenanu, and other destination cities), and (iv) population of at least 1000 households per village. Within villages, sub-villages (Rukun Warga's (RWs)) were selected randomly, and within these RWs, every household was listed until 185 households per village were reached. Of the 869 households sampled, 855 consented to be interviewed, of which 775 met the eligibility criteria.

A.2 Cropping calendar and timing of data collection activities

2 shows the cropping calendar of various crops in West-Timor, Indonesia. Maize and rice are staples. There are some paddy fields, but most crops are grown on dry fields. Rural work opportunities are scant from May until November. West-Timor being predominantly of Christian, most seasonal migrants return to their village of origin at the end of the calendar year for Christmas. The hungry season runs roughly from December until February, before the maize harvests that take place around March. Figure 2 also shows the timing of the intervention and data collection: baseline data collection, treatment (subsidy) offers, migrant tracking, and endline survey.

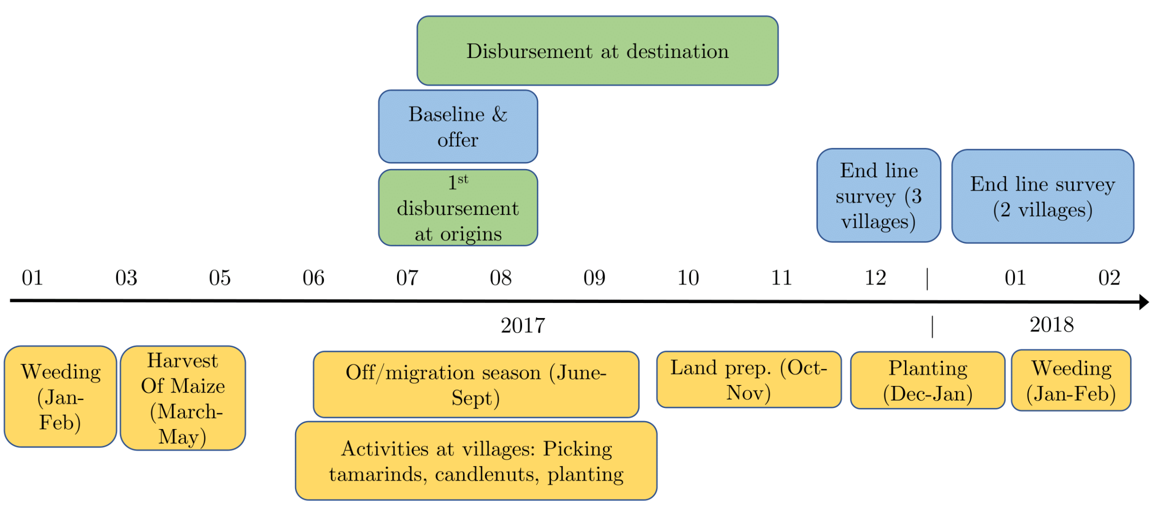

Figure 2: The agricultural cycle in West-Timor and the timing of the baseline and endline surveys.

A.3 Timing of migration

Figure 3 shows that September is the month in which most (as in: more than any other month) of the households have a household member who has migrated and is at a destination. We therefore use household income in the first two weeks of September as out measure of migration season household income.

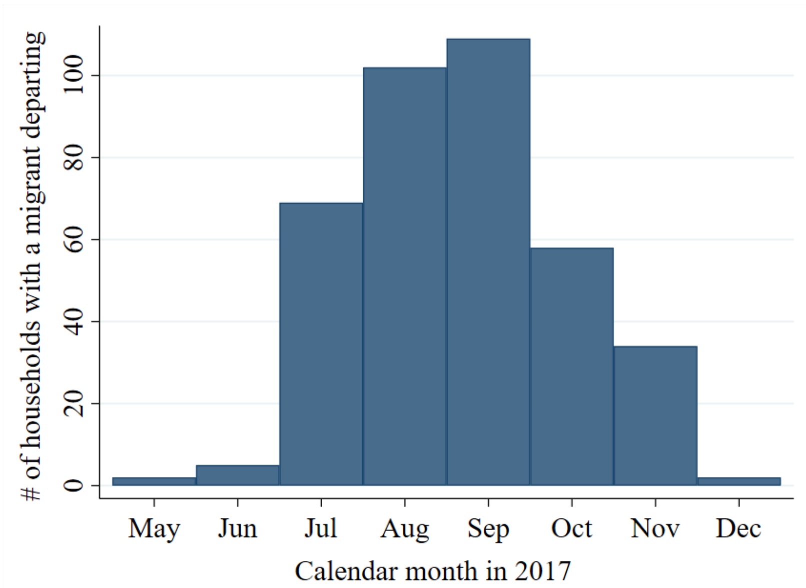

Figure 3: The distribution of the calendar month in which the migration reportedly started.

Appendix B Variable construction

B.1 Migration measures

The binary measure of migration is whether a migrant has checked in at a destination with a program officer. Besides this measure of the extensive margin of migration, there are two measures of the intensive margin of migration: a households' number of migrations since the baseline survey, and the number of different individuals within a household who (have) migrated. Due to time constraints, we only measured up to a maximum of 2 migration episodes: the (time-wise) first migration since mid-July, and the 'last' migration until the endline survey. Of course, migrations were included regardless of whether the migrant had returned by the time the endline survey arrived. Only 16 households had two migrations, of which 7 households had the same individual making more than one trip. Due to this low number of households with more than 1 migration - and the even lower numbers by treatment arm, we do not report experimental results on those outcomes.

B.2 Other outcomes measures: household income and food security index

The income streams over this period, both cash and in-kind, are aggregated over the three biggest income earners in the household. We also have this variable recorded for the week prior to the households' interview at endline, which took place between November 2017 and January 2018. For each income source, the respondent(s) were asked whether this income was earned at the origin or at the destination. Hence, by adding up only those income flows that accrued at the origin, we have a proxy for household income earned at the origin; likewise, we have a proxy for household income earned at the destination by adding up income flows earned at the destination. To ameliorate the unduly influence of outliers, we winsorize all these outcome at the 99^{th} percentile. Apart from household income, the endline survey also asked migrants (or informants about them), to give an estimate of their entire income (cash and in-kind) over the span of their migration. To attempt to get a measure of earnings, this number could be divided by the duration of the migration. Respondents were asked at endline about the start and end date of the migrations episodes. However, for 121 out of 239 households with a seasonal migrant (51%), the respondent could not recall the start date, and so the migration duration cannot be calculated more precisely than in months.

The food security index is constructed using empirical weights on the following questions/variables elicited at endline:

- Compared to the normal portions and normal frequency of HH members who are 17 years old and above normally eat, how many days [0,7] in the last 7 days did those HH members limit meal portion size due to lack of food? (First respondent is asked the corresponding binary question. We imputed 0 here if answered 'No' to the corresponding binary question.) This number of days is divided by the number of HH members aged 17 and above.

- Compared to the normal portions and normal frequency of HH members who are 17 years old and above normally eat, how many days [0,7] in the last 7 days did those HH members have to skip meal because there was not enough food? (First respondent is asked the corresponding binary question. We imputed 0 here if answered 'No' to the corresponding binary question.) This number of days is divided by the number of HH members aged 17 and above.

- In the last 7 days, how many HH members who are 17 years old and above in your household did eat rice?
- In the last 7 days, how many HH members who are 17 years old and below in your household did eat rice?
- How much was total quantity of rice your HH consumed during the last 7 days? Enumerator convert the measurement unit in kilogram
- Did members of your HH consume maize in the last 7 days?
- Did members of your HH consume cassava in the last 7 days?
- In the last 7 days, did your household eat meat or fish?
- In the last 7 days, how many HH members who are 17 years old and above in your household did eat eggs?
- In the last 7 days, how many HH members who are 17 years old and below in your household did eat eggs?

B.3 Control variables

In Table 3 in the main text, as well as in the experimental results in Tables 5-8, we report intent-to-treat (ITT) estimates both with and without the inclusion of control variables in those regressions. The set of controls: the (i) gender and (ii) years of education completed by the household head, (iii) household size, (iv) a PCA-based SES index, (v) whether the interview was administered in Indonesian or in another (local) language, and (vi) a set of indicator variables for all but one of the five sample villages. The indicators making up the SES index include: whether there is a wage earner in the household, land size, housing condition, whether the household was able to save any maize seeds for the planting season, and the numbers of pigs, cows, goats and horses owned by the household.

Appendix C Descriptive results

C.1 Descriptive statistics and balance check

Table 4 reports summary statistics for post-treatment outcomes and balance tests for covariates. The randomization, which was done at the household level, was not stratified on covariates. It appears important to control for household size and baseline socio-economic status (SES) in our intent-to-treat (ITT) estimations, given that the means of those variables are marginally statistically significantly different across treatment arms.

C.2 Reported main usage of the subsidy

Figure 4 shows that the CCT treatments led households in these treatment groups to be substantially more likely to spend the plurality of their subsidy on transportation and accommodation for seasonal migration.
A possibility is that the larger subsidy of the CCT high arm enable lumpy investment opportunities (other than migration) in the origin with a long gestation period, which do not show up (to the same extent as seasonal migration investments) in migration season income. Such lumpy investments (education, fixed capital business investments such as livestock) may help poor households escape poverty traps where those exist (Carter and Barrett, 2006; Giesbert and Schindler, 2012; Balboni et al., 2019; Banerjee et al., 2019). While we cannot rule out this possibility, there do not seem to be substantial differences across the three CCT arms in terms of households' reported primary usage of the subsidy (Figure 4).

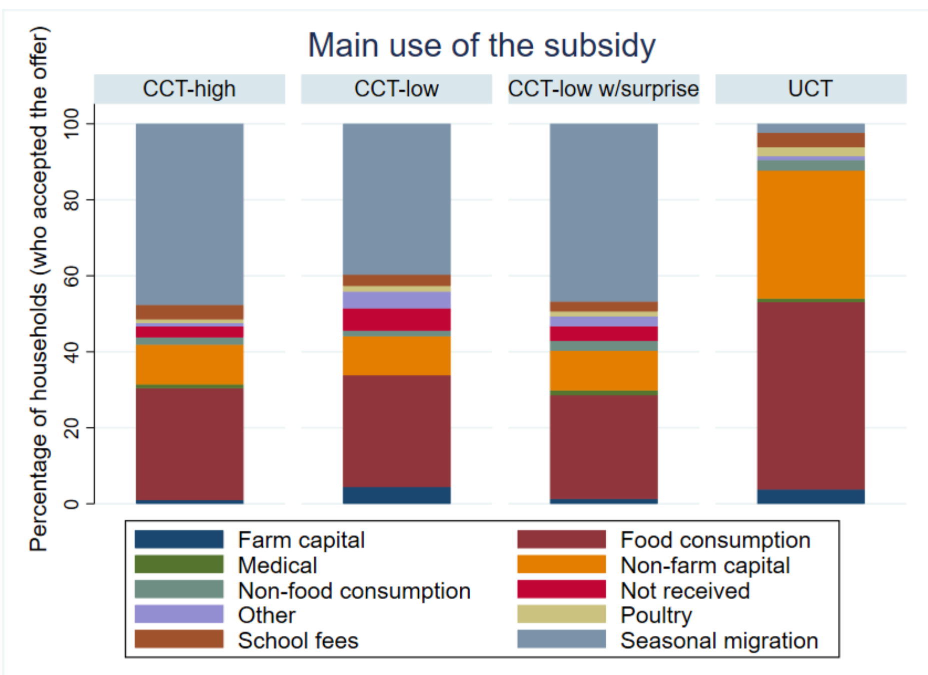

Figure 4: Main use of the subsidy by treatment arm (subsample who accepted the treatment offer).

C.3 Reported migration travel expenditure across treatment arms

Figure 5 plots the empirical CDF of travel expenditure of the migrants in each of the treatment arms. It does not reveal systematic differences between the CCT arms in terms of travel expenditure to the destinations.

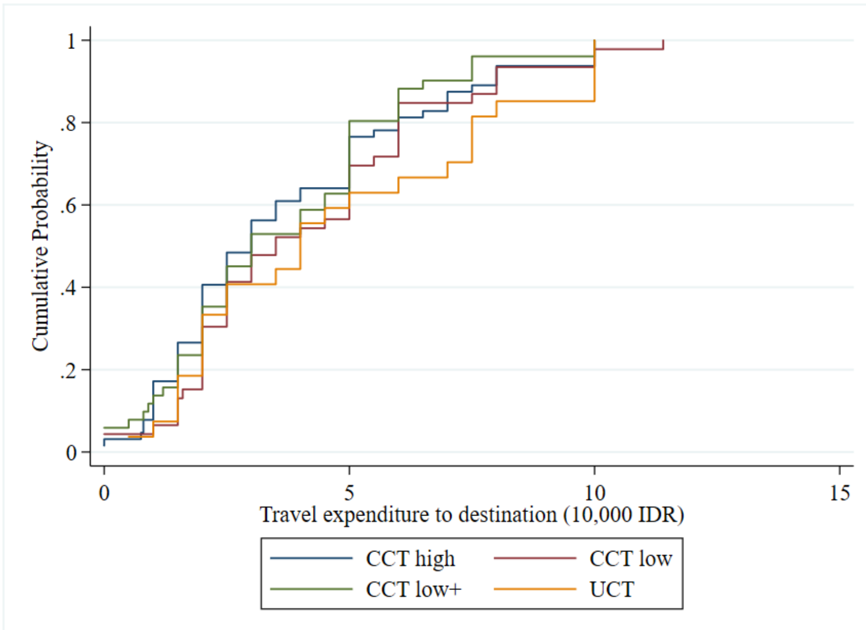

Figure 5: The empirical cumulative distribution functions (CDFs) of migrant travel expenditures across treatment arms.

Table 4: Summary statistics and randomization checks.

	Full sample		CCT high	CCT low	CCT low+	UCT	
	Obs.	Mean (st. dev.)	Mean (st. dev.)	Mean (st. dev.)	Mean (st. dev.)	Mean (st. dev.)	p-value
Primary outcomes							
Accepted offer	775	0.66 (0.48)	0.49 (0.5)	0.46 (0.5)	0.44 (0.5)	0.85 (0.36)	0.000
Checked in at dest.	775	0.17 (0.37)	0.27 (0.45)	0.17 (0.38)	0.19 (0.4)	0 (N/A) (0)	0.020
Secondary outcomes[2]							
Employed (i.e., not idle)	708	0.9 (0.3)	0.94 (0.24)	0.89 (0.31)	0.9 (0.3)	0.87 (0.34)	0.112
Salary work	708	0.22 (0.41)	0.26 (0.44)	0.24 (0.43)	0.24 (0.43)	0.16 (0.37)	0.066
Income in Sept. (10k)	708	113.16 (177.31)	102.03 (148.09)	140.7 (245.73)	136.29 (198.9)	90.51 (124.66)	0.015
Food security index	686	0 (1)	-0.08 (0.97)	0.01 (1.06)	0.02 (1.04)	0.04 (0.97)	0.698
Covariates[3]							
Female headed HH	775	0.19 (0.4)	0.17 (0.38)	0.18 (0.39)	0.17 (0.38)	0.23 (0.42)	0.323
HH size	708	4.27 (1.81)	4.31 (1.57)	4.14 (1.66)	4.58 (1.89)	4.12 (1.98)	0.077
Speaks Indonesian	708	0.83 (0.37)	0.83 (0.38)	0.9 (0.3)	0.81 (0.39)	0.82 (0.39)	0.146
SES index	775	-.02 (0.94)	0.04 (0.91)	0.07 (1.01)	0.03 (1.05)	-0.1 (1.02)	0.099
Education (years)	708	6.48 (3.76)	6.45 (3.57)	7.05 (3.94)	6.37 (3.93)	6.23 (3.65)	0.474
Attrition							
Re-interviewed	775	0.91 (0.28)	0.91 (0.29)	0.88 (0.33)	0.92 (0.28)	0.94 (0.24)	0.195

[1] For categorical variables, the p-value of the equality across treatment arms is based on a χ^2 test; for continuous variables, it is based on ANOVA. The 'checking in' outcome only applies to the CCT arms.
[2] Measured at endline.
[3] Household size, educational attainment of the household head, and the indicator for the household head speaking Indonesian, were recorded at endline.

Appendix D Experimental results

D.1 Treatment offer acceptance

Table 5 reports the acceptance rates of the cash transfer offers across the treatment arms. As expected, most households accept the UCT offer[5]. The difference in acceptance rates among the three CCT arms does not attain statistical significance.

Table 5: Regressions of accepting the treatment offer on the treatment arms (linear probability models).

	(1)	(2)	(3)	(4)
CCT high	-0.368***	-0.342***	-0.368***	-0.342***
	(0.039)	(0.039)	(0.039)	(0.039)
CCT low	-0.418***	-0.390***		
	(0.043)	(0.045)		
CCT low+	-0.419***	-0.413***		
	(0.042)	(0.044)		
CCT low/low+			-0.418***	-0.403***
			(0.032)	(0.034)
Test high=low=low+	0.534	0.318		
Test high=low & low+			0.262	0.186
Constant	0.938***	0.893***	0.928***	0.893***
	(0.016)	(0.057)	(0.016)	(0.057)
Controls		✓		✓
N	775	708	775	708
R^2	0.156	0.183	0.156	0.183

* $p<0.1$, ** $p<0.05$, *** $p<0.01$; robust standard errors clustered at the household level in parentheses. UCT is the left-out category/arm. Controls include the head of household's gender and years of education, whether (s)he speaks Indonesian, household size, and a socio-economic status (SES) index, and village indicators. Details on the outcome variables and the construction of the SES index can be found in Appendix B.

D.2 Differences between the CCT arms in migration aspects

To explore differences in behavior brought about by the high CCT (as compared to the CCT low/low+), we conduct additional exploratory analyses of differences between the treatment arms in terms of aspects of the migration (6). Due to the low absolute number of migrants per treatment arm, few of the coefficients are statistically significant and the difference between the three CCT arms is not statistically significant for any of the estimates. We are unable to

[5] Based on anecdoctical evidence, the reason 7% of UCT-assigned households do not take up the cash transfer offer is distrust, given prior experience with shams.

determine with statistical confidence whether the CCT high opted to migrate earlier (column (1)) or for a longer duration (column (2)), be more likely to migrate to a city (closer to the check-in where to receive the second subsidy) (column (6) or within the district (column (5)), rather than somewhere else where returns may be higher, whether they are more likely, for instance, to send the household head to seasonally out-migrate as opposed to another household member (column (7)), or whether they have a household member who migrated in the past (column (8)).

Table 6: ITT estimates on aspects of migration for the subsample of migrants.

	(1) Migration start month	(2) Migration duration	(3) Transport cost	(4) Total migr. cost	(5) Dest: TTU	(6) Dest: Urban	(7) Head migrated	(8) Previously migrated
CCT low	-0.026	-0.057	-0.886	-1.959	-0.053	0.070	-0.171*	-0.046
	(0.083)	(0.159)	(0.701)	(1.391)	(0.110)	(0.093)	(0.087)	(0.125)
CCT low+	-0.061	0.053	-1.241*	-2.279*	0.119	-0.009	-0.103	0.024
	(0.083)	(0.148)	(0.652)	(1.289)	(0.104)	(0.095)	(0.076)	(0.114)
CCT high	-0.095	-0.068	-0.335	-1.730	0.083	0.050	-0.179**	0.010
	(0.082)	(0.136)	(0.765)	(1.360)	(0.101)	(0.092)	(0.075)	(0.113)
Test (p-value)	0.519	0.514	0.384	0.592	0.163	0.582	0.538	0.775
E[dep. var. | UCT]	2.950	2.309	4.178	3.974	0.510	0.749	0.732	0.663
(st. dev.)	(1.101)	(1.588)	(3.481)	(4.549)	(0.501)	(0.435)	(0.444)	(0.474)
N	238	223	190	239	239	239	239	190
(Pseudo-)R^2	0.005	0.036	0.089	0.264	0.060	0.052	0.106	0.065

* p<0.1, ** p<0.05, *** p<0.01; robust standard errors clustered at the household level in parentheses. The outcome of column (1) takes on 0 if the migration reportedly started in May 2017, 1 if it reportedly started in June 2017, etcetera, and 7 if it reportedly started in December 2017. The outcome of column (2) is the duration of the seasonal migration in months, which is censored at the endline interview month for households whose migrant had not returned by the time of their endline interview. Column (3) (migration transport expenditures) and column (4) (total migration expenditure including housing) are in 10,000 IDR. The sample consists of households in TTU regency, so the outcome in column (5) takes on 1 if the migration destination is within the same regency (and 0 otherwise). Column (6) takes on 1 if the migration is to an urban destination, column (7) takes on 1 if it was the household head (rather than another household member) who migrated, and column (8) takes on 1 if at least one household member had ever migrated before the baseline survey. Estimates of columns (1) and (2) are based on Poisson regressions; those of column (3)-(8) are based on OLS regressions. UCT is the left-out category/arm. Controls include the head of household's gender and years of education, whether (s)he speaks Indonesian, household size, and a socio-economic status (SES) index (details can be found in Appendix B), and village indicators.

D.3 Intent-to-treat effects on employment status

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Table 7 shows estimates on work (any work, including self-employment) and salaried work during the peak of the migration season (the first two weeks of September). The CCT raises the propensity to have work in September compared to the UCT, but there are no statistically significant differences between the three CCT arms.

Table 7: ITT estimates on work and salaried work (i.e., working for others) during peak migration season (linear probability models).

	(1) Work (any)	(2) Work (any)	(3) Salaried work	(4) Salaried work
CCT high	0.072**	0.068**	0.099**	0.095**
	(0.028)	(0.015)	(0.040)	(0.034)
CCT low	0.024	0.027	0.081*	0.071*
	(0.035)	(0.027)	(0.044)	(0.027)
CCT low+	0.034	0.037	0.077*	0.059
	(0.033)	(0.028)	(0.042)	(0.035)
Test (p-value)	0.225	0.084	0.888	0.574
E(dep.var. | UCT)	0.868	0.868	0.158	0.158
	(0.340)	(0.340)	(0.366)	(0.366)
Controls		✓		✓
N	708	708	708	708
R^2	0.008	0.023	0.010	0.041

* p<0.1, ** p<0.05, *** p<0.01; robust standard errors clustered at the household level in parentheses. UCT is the left-out category/arm. Controls include the head of household's gender and years of education, whether (s)he speaks Indonesian, household size, and a socio-economic status (SES) index, and village indicators. Details on the outcome variables and the construction of the SES index can be found in Appendix B.

D.4 Intent-to-treat effects on food security

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Table 8 shows ITT estimates on the food security index.

D.5 Empirical CDFs of migration season household income

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Figure 6 shows the empirical CDFs of migration season household income (in IDR 10,000) across treatment arms.

D.6 Spillover effects

Since randomization into treatments was done at the household level, it is possible that information about seasonal jobs spilled over from CCT-assigned households to some UCT-assigned households who reside in the same village, or that UCT-assigned households are induced by CCT-assigned households in their village to co-migrate with them. As a thought experiment, intra-village CCT-to-UCT spillovers - regardless of the mechanism through which they operate, would be zero for UCT-assigned households if no households in the

Table 8: ITT estimates on the food security index at endline.

	(1)	(2)
CCT high	-0.114	-0.096
	(0.097)	(0.083)
CCT low	-0.028	-0.059
	(0.111)	(0.162)
CCT low+	-0.013	0.012
	(0.107)	(0.086)
Test (p-value)	0.620	0.436
Constant	0.038	0.310**
	(0.064)	(0.107)
Controls		✓
N	686	686
R^2	0.002	0.054

* p<0.1, ** p<0.05, *** p<0.01; robust standard errors clustered at the household level in parentheses. UCT is the left-out category/arm. Controls include the head of household's gender and years of education, whether (s)he speaks Indonesian, household size, and a socio-economic status (SES) index, and village indicators. Details on the outcome variables and the construction of the SES index can be found in Appendix B.

village would receive the CCT treatment (as would be the case with village-level randomization). Hence, we assess heterogeneity in the ITT on household income by the number of other households in the village that received a CCT treatment (which has some random variation). While not statistically significant, Figure 7 shows suggestive evidence of modest spillover effects.[6] Since the impact on migration season income of the CCT compares favorable to that of the UCT, the negative slope of the conditional ITT in Figure 7 implies that the ITT estimates on migration season income of CCT (compared to UCT) reported in Table 3 in the main text, if anything, are lower bounds on the corresponding true effects.

[6]The doubly robust conditional average treatment effect estimator by Lee et al. (2017) was used.

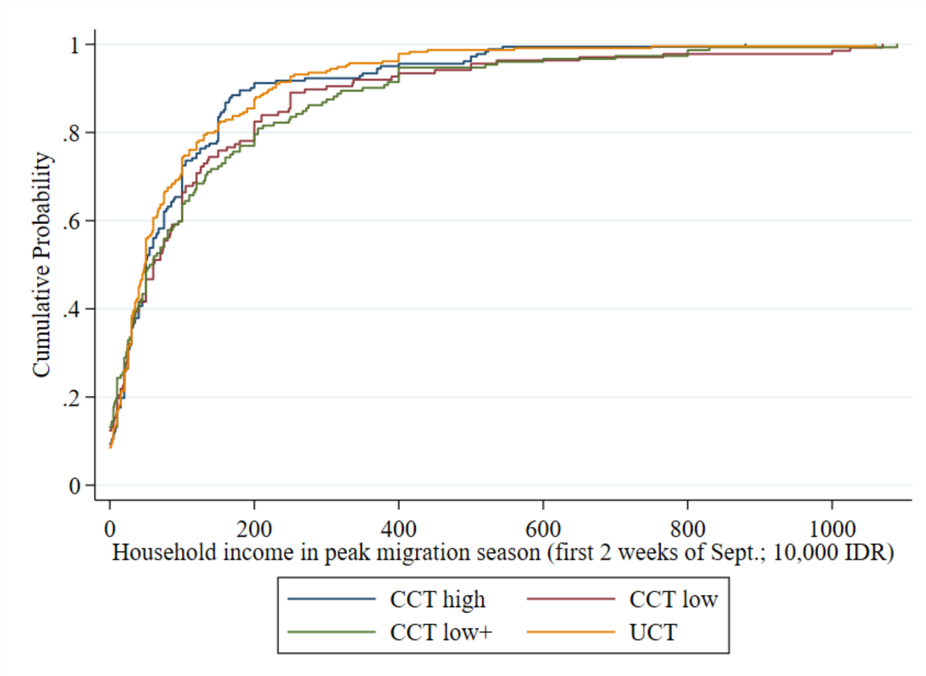

Figure 6: The empirical cumulative distribution functions (CDFs) of household income (in IDR 10,000) by assigned treatment. Note: the plot region was limited to Rp. 11,180,000; only one observation (in the CCT low group) of Rp. 19,560,000 exceeds that value.

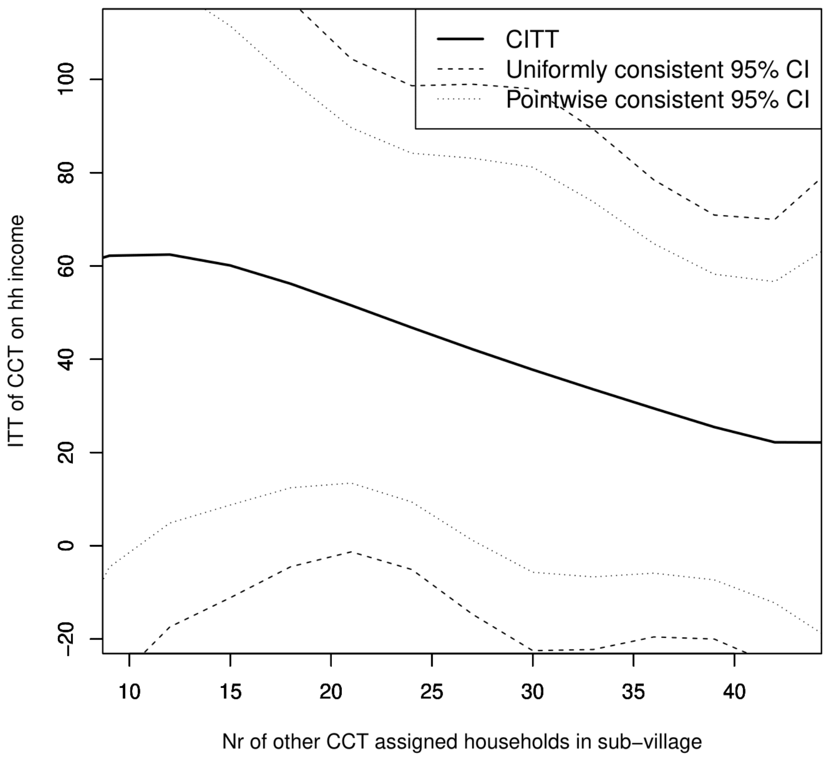

Figure 7: Household income by assigned treatment. CITT stands for Conditional Intent-to-treat effect.